

caused by the caloric evolved by the transition of the aqueous vapour of the breath into the liquid or solid form.

Before seeing Dr. Roberts's explanation I referred the matter to the greatest living authority on heat, and he, after carefully repeating my experiments, was of opinion that the heat was produced by the compression of the air when forced through the material. Had he known of Dr. Roberts's simple but ingenious variation of the experiment there is no doubt he would have accepted Dr. Roberts's explanation. R. E. DUDGEON

November 18

THE following experiment may serve to supplement the observations of Dr. Roberts as to the cause of the high reading of a thermometer wrapped in a handkerchief and placed in the mouth. An ordinary non-registering thermometer was wrapped in about twelve folds of a dry linen handkerchief placed in the mouth, and the following readings taken at intervals of one minute:—Inspiration was effected through the nostrils, expiration through the handkerchief. The thermometer was in the mouth from the beginning to the end of the experiment. Temperature under the tongue before commencing, 37° 0 C. The reading of the thermometer wrapped as above described, one minute after introduction into the mouth, was 43° 0. At the end of the second minute, 44° 1, 3rd 42° 9, 4th 41° 2, 5th 39° 6, 6th 38° 2, 7th 37° 1, 8th 36° 9, 9th 36° 9, &c. After the experiment the temperature under the tongue was 37° 6. Capillarity is probably the chief cause of the rapid condensation of water, and the consequent liberation of heat in the dry fabric.

In connection with the above I may mention a schoolboy's trick, viz., gripping the arm of a schoolfellow with the teeth and breathing forcibly through his coat-sleeve. The sensation of heat thus produced is much greater than when the breath is allowed to impinge on the bare skin.

In conclusion, I must freely confess that Dr. Dudgeon completely upset my objection, as to compression of the bulb having anything to do with the high reading, by the experiments quoted in his last letter. F. J. M. P.

Coral Reefs and Islands

IN my letter on "Coral Reefs and Islands," published in NATURE, vol. xxii. p. 558, I have just noticed an important slip in writing which demands correction.

In the third paragraph and ninth line, for *metres* read *miles*, so that the passage shall read thus: "On the Florida coast we have barriers with channels 10-40 miles wide."

More accurately, the space between the southern coast of Florida and the line of Keys (old barrier reef) gradually widens from a few miles in its eastern to more than 40 miles in its western part. The channel between the line of Keys and the present reef is 6-7 miles wide and about 150 miles long.

Berkeley, California, November 2 JOSEPH LeCONTE

Vox Angelica

I HAVE received a letter from Mr. Samuel Ray of Stoke Newington with reference to my remarks on the Vox Angelica slip on an Estey American organ. Mr. Ray informs me that Gordon's supplementary tuning-valve is used for the desired effects. The rationale of the method is, that by partly closing the mute the reeds are flattened, just as one reed is when the key is partially depressed. Mr. Ray also says, that by pulling out the stop a little way and making the reeds beat the latter are liable to be drawn out of tune; but this was the original method, but is now improved upon. A separate mute is placed on the top of the tubes, so that the wind strikes one of the sets of reeds vertically, whereby undue strain is avoided.

GEORGE RAYLEIGH VICARS

Woodville House, Rugby, November 18

Fascination (?)

PROBABLY none of your readers have thought it worth while to make any comment on the letters on this subject which have recently appeared, because it would seem needless to discuss the origin of "fascination" by means of the eye of a snake (or whatever may be the stimulus to the alleged condition) while all the evidence we can obtain from these reptiles in confinement proves that the condition does not exist. It devolves upon those who might object to observations on reptiles in a glass case as

untrustworthy, to show us why—all their other actions being normal—the prisoners should not exhibit the same habit in respect to this "fascination," as they are alleged to practise when free. It is rather late in the year now; but if Mr. L. P. Gratacap will take the first opportunity of seeing snakes feed, and if any of your readers will pay a visit to the Zoological Gardens, both he and they will, I think, come to the conclusion that, beyond the expression of a little surprise (on the part of ducks and pigeons chiefly) which soon wears off at the sight of an unfamiliar object, both the birds and animals regard the snakes with marked unconcern. I have seen a guinea-pig, after finding no place of exit from the cage, quietly settle itself down in the midst of the coils of an Australian constrictor, shut its eyes and go to sleep. Ten minutes afterwards the snake had moved, and the guinea-pig was washing its face with its paws. Not once, but a dozen times, a rabbit has nibbled the nose of a River Jack viper (*V. rhinoceros*) in a pretty, inquiring way, heedless of the strong blows the reptile would administer with its snout to the impertinent investigator of that queer-looking object. For fully ten minutes one day a rabbit sat gazing at the poised and threatening head of a puff adder, now and then reaching forward to smell the reptile's nose, and anon sitting on its hind legs to wash its ears, and again returning to the "fascinating" object of its inquiries. If during that time the rabbit had fallen into the state of trance, it was so soon released from that condition as to be able to attend to its own comfort and busy itself about its toilet. The birds show no more recognition than the other animals of the dangerous position in which they are placed. We see them hopping about on the snakes and pecking lustily at their scales; sitting on the branches, preening their feathers and behaving themselves just as though no such dreadful (or pleasing?) sensation as "fascination" was possible!

I saw once a sparrow perched upon the body of a snake twisted round a branch, and preening itself. By-and-by a constrictor crept up slowly, touched the bird with its nose, and then threw the crushing folds around it. The deliberate approach of the snake and the unconscious attitude of the sparrow, concerned about its private affairs, would have staggered any ordinary believer in "fascination." I have closely watched the behaviour of snakes intent on feeding. It may be a sudden rush, when the victim has no time to see its enemy, or the gradual, lazy advance of the reptile; in either case the doomed victim betrays no suspicion of danger—at least so far as I have been able to ascertain after passing some hundreds of hours contemplating the snakes in the unequalled representative collection of the Zoological Society.

The expression in Mr. Gratacap's letter, "glittering" eyes, applied to the orbits of a snake, which are veiled by the "antocular" membrane, and capable of very slight movement, may remind us of Virgil's "Suffecti sanguine et igni," and help to confirm the "basilisk" (not a snake, by-the-by) superstition, but can only serve to perpetuate a myth. Whatever may be the value of Mr. Foot's opinion, I would ask, "Who has ever seen a snake 'raise its tail' after the manner of the cats?"

Charles Darwin has much to say on this subject to any one who chooses to consult the "Origin of Species." He does not see any advantage in the cat's "waving" tail or the noise of the "rattle" of *Crotalus*, for no predatory animal would derive any benefit from a signal of warning to its prey. The snake certainly never "waves" its tail when intent on mischief.

ARTHUR NICOLS

Soaring of Birds

REFERRING to NATURE, vol. xxiii. p. 10, may I suggest the following?—The question seems to be: "How can birds, having attained a certain elevation, thence rise without further muscular effort?" If I am not in error in what follows, they can theoretically do so if they start with a difference between their horizontal velocity and that of the wind, and end with a less difference; e.g., if they start at rest with respect to the earth, and end by drifting with the wind entirely.

Take this last case, and consider the air as plane, and the wind as horizontal, and having a velocity = v with respect to the [earth and] bird. Finally we suppose the bird gains a horizontal momentum = mv . Then, by conservation of horizontal momentum, the only force acting being vertical, the air must lose an equal horizontal momentum.

Now we know that in all cases of bodies colliding and ultimately acquiring the same velocity, while we have conservation

of momentum we have loss of visible kinetic energy, except when the coefficient of restitution = 1. This kinetic energy is transformed into the vibrational kinetic energy of sound and heat in general.

But cannot we have it partly transformed into potential energy by "soaring" against gravity? On this supposition we have the two laws, conservation of momentum where no forces act, and conservation of energy, holding. But we have visible kinetic energy lost and *partly transformed into potential energy with respect to the earth*, partly (as usual) into vibrational kinetic energy of sound and heat. [The sound is evident in the "singing" of the wings.]

It seems to me that the swooping referred to by your correspondent is only a matter of convenience to the bird, and does not really affect the mechanical question; and that the comparison to a kite (which is held by a string) is not very satisfactory. But from my own observation of sea-gulls I do not think one can say that all the manoeuvres and turns of the bird in the air are performed without real muscular effort, though certainly without *flaps* of the wing; and if there be muscular effort there can be work done—against gravity in this case.

The above is only a suggestion. I wish to induce some more mathematical reader to write a clear answer on this interesting question.

W. LARDEN

Cheltenham, November 8

The Photophone

ON reading the description published in NATURE, vol. xxiii, p. 15, of Prof. Graham Bell's wonderful discovery, the transmission of speech by light, I notice that in "the photophone" the varying of the intensity of the beam of light thrown on the receiving instrument is accomplished by the simple and ingenious means of allowing the sound-waves to beat on the back of a thin plane mirror. It seems to me, however, that this arrangement is not complete, and is open to some objection. As the plane mirror will, if provision be not made against it, become convex and concave alternately, it must, unless the vibrations be confined within very narrow limits, give in one vibration *two* periods of maximum and minimum illumination at the receiver, and therefore the received sounds, apparently, should be (assuming the periods between each maximum and minimum illumination to be of the same duration, which could never exactly occur) an octave higher than those transmitted. This I think follows from the fact that the rays from the mirror would be dispersed not only when convex, but also when concave, *after* they had passed the focus. If, therefore, the vibrations of the mirror are sufficiently great to bring its focus between the mirror and the receiving instrument, there would be a second point of minimum illumination. If however the mirror were made slightly convex, or were constrained by a spring or otherwise, this defect would be cured.

Curiously enough, *theoretically* "the photophone" is the more effective the greater the distance between the transmitter and the receiver, as the degree of variation of the intensity of light falling on the selenium will be, when perfectly adjusted, greater as the distance increases, and it is on this element that the intensity of the sound depends.

A. R. MOLISON

Ffynone Club, Swansea, November 15

[Our correspondent is obviously right in supposing that with a beam of light focussed accurately upon the selenium receiver a *single* complete vibration of the transmitting disk would produce *two* periods of maximum and minimum illumination. This would not however be the case if the lenses were not set originally to exact focus, for then a displacement of the disk in one direction would scatter the rays more, while a displacement in the other would concentrate them more. In practice, we believe, exact focussing is never obtained or even attempted.—ED.]

Salts of Zinc

IN Roscoe and Schorlemmer, vol. ii, p. 264, it states: "The salts of zinc do *not* impart to the non-luminous flame any tint;" and on p. 258, "the metal burns with a bright *white* flame."

What then is the green colour imparted to the Bunsen flame by zinc sulphate due to? Also the green flame obtained by heating metallic zinc on charcoal before the blowpipe? S.

THE green tint referred to by "S." (*supra*) as imparted by zinc sulphate to the Bunsen flame is only observed whilst the water of

crystallisation contained in the salt is being given off; the dry salt which remains imparts no colour to the flame. It therefore appears probable that the green colouration of the flame is caused by very finely divided particles of the salt being carried off into the upper part of the flame by the escaping water of crystallisation. These particles then become so intensely heated as to emit the peculiar greenish light and very likely suffer previous reduction by the carbon of the flame. Other zinc salts, especially the acetate, impart to the flame, when first heated, a greenish-blue tint resembling that observed when metallic zinc is burnt in the air, this being doubtlessly due to a partial reduction of the acetate. The characteristic zinc lines (6362 and 6099 in the red, and 4928, 4924, and 4911 in the blue) are not seen in the case of the salts or when the metal is burnt. A more correct description of the combustion of zinc than that referred to would be: "the metal burns with a bluish-white flame."

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W. BOTT

THE WORKS OF CARL VON NÄGELI

THE beginning of the forties in the present century marks an important epoch in the history of botany. The "Naturphilosophie" which had for many years so banefully influenced the development of the science, was being routed by the energetic attacks of Schleiden. Botanists were becoming alive to the fact that if their study was to have a place as a science by the side of physics and of chemistry, it must be pursued by the inductive method; that speculation must give way to research, and, above all, that development must be studied before any conclusions could be drawn from the investigation of mature forms. The early discoveries of von Mohl, and the demonstration of the cellular structure of the tissues by Schleiden, were among the first fruits of this awakening. To this period belongs also Nägeli's first contribution to science—a paper on the Development of the Pollen (1842). The first sentence in the introduction shows how thoroughly Nägeli was imbued with the same spirit which possessed Schleiden. He says:—"The right knowledge of an object includes an acquaintance with its mature form and a study of its development: the one is dependent upon the other, and the one without the other is insufficient to afford a complete conception of the object." The actual observations detailed in the paper appear from the drawings to have been accurate, and they were an important addition to the knowledge of the subject; but their interpretation was so far influenced by Schleiden's theory of cell-formation, which was then prevalent, that the process of the development of the pollen grains is described as being one of free cell-formation.

In the year 1844 appeared the first number of the *Zeitschrift für wissenschaftliche Botanik*, edited—probably on account of the sympathy existing between them—by Schleiden and Nägeli. This short-lived periodical (1844 to 1846) was practically an organ for the publication of Nägeli's researches and for the expression of his views, for it does not contain a single contribution from Schleiden's pen. The first number opens with an article—a sort of confession of scientific faith—"On the Present Aims of Natural History, and especially of Botany," in which he gives an account of the actual state of botanical knowledge, and strongly urges the necessity of empirical study in order that the generalisations of the science might be in the future, not baseless speculations, but inductions resting upon a firm foundation of ascertained fact. The *Zeitschrift* further contains an important paper "On the Nuclei of Cells and the Formation and Growth of Cells," in which the process of free cell-formation, which Schleiden had asserted to be universal, is shown to be only one of the processes by which a multiplication of cells is effected; these processes are clearly defined and classified. This is followed by a number of researches on the morphology of the lower cryptogams, which are of interest inasmuch as they open up new lines of approach to the study of the complicated morphology of more highly